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"stamps in" the act which produces it. The difficulty lies in not understanding how an act can be emphasized by a successful result which follows it and which must, therefore, have a backward influence upon it. If responses overlap, forming larger unities, is it, then, not conceivable that no "act" is complete, in the higher organisms at least, until all responses of its own complex—*e. g.*, those directed toward getting out of a problem box—find expression together? May there not be mutual inhibition and delayed response until some consistent outlet involving mutual reinforcement is found by the trial and error processes? It is hoped that this hypothesis will tend to put the explanation of learning on a more objective and experimental basis than the conventional one of mere neural processes, often vaguely conceived or wholly hypothetical.

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THEORY AS TRUTH: A STUDY OF THE LOGICAL STATUS OF SCIENTIFIC THEORY[1]

> "Although in this long journey we miss the intended end; yet are there many things of truth disclosed by the way; and the collaterall verity may unto reasonable speculations some what requite the capital indiscovery."[2]

THE greatest feats of science to-day—for science is the acquisition and communication of knowledge—are, I suppose, not its facts, much less its deeds, but in very truth its theories. The theories of modern science deal commonly with things that we can never hope to reach with our senses, with matters that are beyond the reach of immediate experience. To questions which have been under discussion through the ages, questions such as those of the atomic constitution of matter and the nature of light, science to-day in its theories gives perfectly definite answers. The nature of the case forbids that these questions shall be answered by facts. When the theories which answer them, however, are submitted to the scrutiny of logic, it appears that they are not hypotheses in any sense and that they have a certainty of the same order as that of *any bit of experience.* It is as certain that matter consists of atoms as it is certain that experiments can be performed in the laboratory—and the opinion that matter is made up of atoms is no guess: we may be as sure of it as we are sure that there is matter at all.

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[1] The notions here developed were first worked out for Professor Royce's seminary, 1914–15.

[2] Sir Thomas Browne, "Pseudodoxia Epidemica," Book VI., Ch. 12.

Unfortunately, the scientists themselves use the word, *theory,* loosely and without any exact conception of its application. One says: "A theory makes it possible to understand and remember an extraordinary number of facts with little effort. A theory is a short way of describing a group of phenomena."[3] "A theory," says another, "is a supposition we hope to be true; an hypothesis is a supposition which we expect to be useful."[4] Still another believes a theory to be an especially plausible kind of interpretation.[5] It seems unnecessary to multiply instances. The loose use of the word is evident.

Law has been defined for science, and the inductive method has been studied very thoroughly by logicians, notably by Charles Peirce. The use of hypothesis as a stimulator of research and a maker of *law* has been described; and hypothesis has been exalted to the highest by pragmatism. Theories, so far as I know, have not been defined. The need of a definition is all the more crying inasmuch as there are some "theories" in science which are neither laws nor hypotheses. Some of the so-called "theories" are actually hypotheses, some of the so-called "hypotheses" belong to this third class of knowledge and, although *workable,* they possess a certainty wholly beyond that inherent in their workableness. Hypothesis is *possibly* true, law is *probably* true, and *theory* is as true as a fact. "What?" said Dr. Johnson, in talking about the metaphysics of Berkeley, "to tell me that *that,*" here he kicked the table-leg, "doesn't exist"? Whatever may be the merits of the arguments for the existence of *that,* if you grant that it exists at all, then you grant at the same time that it consists of atoms—or, if we only dream *that,* then, in a world where there are such dreams, there is only one correct way to dream—which is that *that* consists of separable particles.

In this paper, therefore, I shall attempt to point out that there exists, among the mass of knowledge which constitutes science, a certain class of opinions which are logically different from laws and hypotheses. Many of them are already called "theories." For all members of the class I propose the name *theory.* I shall show that theories possess a special kind of certainty. They are the surest thing that science knows.

I. Instances of Theory and of its Scope

In 1849 Fizeau[6] performed an experiment in which a beam of light was made to pass between the teeth of a toothed wheel to a distant mirror from which it was reflected back again over the same path to the eye of an observer. As the wheel was rotated, a beam of intermittent light passed through to the eye. When the speed of the wheel was increased, a point was reached at which the light was totally eclipsed and none passed through to the eye. That is, during the time required for the light to pass between the teeth to the mirror and back again to the place through which it came, during that time the wheel had moved so that the point in space occupied by the opening between the teeth was now occupied by the tooth itself, and no light could get through. At twice the speed of rotation required for this first eclipse the light again passed through to the eye. The light passed through one notch in the periphery of the wheel to the mirror, whence it was reflected back to the wheel, now to pass through the notch next the one through which it came. At three times the original speed the second eclipse occurred, and so on.

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[3] Daniel Comstock.

[4] G. J. Stoney.

[5] Theodore Richards.

[6] Fizeau's experiment was not the first consideration to indicate the space-time quality of light. I have selected it because it seems clear and suitable for the present purpose.

Although in describing this experiment I have been forced to speak of the time required for light to travel from one place to another, it is still evident that the experiment might be observed without any foreknowledge (or after-knowledge, for that matter) of the velocity of light. All that is seen is an arrangement of mirrors and lenses, a rotating toothed-wheel, and sometimes light through the eye-piece, sometimes not, according to the rate of rotation of the wheel. But, *unless light is something which has a velocity,* these things could not be observed. That is the theory. It is *true,* as the older physics books used to say, that light is a "mode of motion."[7]

Theories, when discovered, usually have the effect of destroying some hypothesis which was held earlier. Descartes guessed that light was "the effect of a pressure instantaneously transmitted through a universal medium."[8] The fact that light has a velocity means that light can not be the effect of a static pressure. When it became clear that light was a mode of motion, two rival hypotheses sprang up—the emission or "corpuscular" hypothesis of Newton and the undulatory hypothesis of Huyghens. Both have always been called "theories," but both were pure guesses, made because they offered what seemed to be plausible explanations of certain phenomena. There was a little truth: these, in addition, supplied gratuitous opinion. I know no better name for them than the one used by William James in discussing religious experience. They were "over-beliefs."

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[7] Unfortunately theories are usually expressed as universal statements. The real theory, which is true, is that light (*the particular light which was used in the experiment*) has a velocity. The statement that light (*all light*) has a velocity is something else—law or convenient definition, I do not know—and, of course, the statements here made about the truth in theories do not apply to that universal statement. In the following discussion I shall express certain theories in the form of universal statements, for such is the habit of science. But real theories are, in strict logic, particular statements—as particular as the experiment.

[8] Duff, "A Text-book of Physics," page 360.

The hypotheses of Newton and of Huyghens seemed to explain about equally well the phenomena of reflection and of refraction. The discovery of interference eliminated one of them. Some thinkers in the methodology of science have called the experiment of Thomas Young on interference a "crucial experiment"—because it decided between two rival hypotheses. It is that, but it seems to me that there is something about it far more important. *Even if there had been no Newton and no Huyghens, the experiment of Young,* properly interpreted, *would have led to* a certain theory, a certain truth—*the undulatory theory of light.*

In Young's experiment, light from a single source is made to pass through "two narrow slits, very close together, which act as secondary sources. If a screen be placed beyond these slits a series of colored and dark bands parallel to the slits will be observed on it. If one of the slits is covered, the bands disappear. This shows that they are the resultant effect of two superimposed pencils of light alternately reenforcing and destructively interfering with each other."[9] Unless light is a wave motion, there could be no reenforcement and interference, and the screen, placed beyond the slits, would be evenly illuminated in all its parts. Given this observation, it becomes certain that light is a wave motion. That is the theory.

It is to be noticed that a specific fact, or law, or set of laws, renders certain such and such a truth—but there is still room for over-beliefs. Another law is discovered which renders certain a little more of the truth and does away with part of the over-belief. When enough laws have been accumulated the situation becomes closed—within this small area we have truth—within these limits there is nothing more to be said. Light is a mode of motion—the area is mapped out. Light is a wave motion—there is no more truth obtainable within this area. The result of this is that the theory is necessarily broad in its phraseology. It does not go into details. That which does go into details and which may be expressed exactly is the law—which is probable.

Boyle's law states that, the temperature being constant, the volume of a gas varies inversely as the pressure. Charles's law states that, volume being constant, the pressure of a confined volume of gas varies directly as the absolute temperature, or, that, pressure being constant, the volume varies directly as the absolute temperature. The two laws may be combined, thus: $PV = KT$. It is probable that the pressure-volume product for a confined quantity of any gas is directly proportional to the absolute temperature. *Unless* gases are alike in their inward constitution this could not be. It is true then that gases are *mechanically* alike. That is the beginning of a gas theory.

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[9] Duff, *loc. cit.*, page 364.

Gay-Lussac's law says that gases combine chemically with one another volume for volume or in proportions by volume which are expressible as the ratios between small whole numbers. Thus, one volume of hydrogen combines with one volume of chlorine to form hydrochloric acid, one of oxygen combines with two of hydrogen to form water. There is also the law that elements combine with one another in definite ratios by weight. On the strength of this second law a *chemical unit* (an atom) may be defined as that quantity by weight of a given element which will combine with a unit of some standard element—with a unit of hydrogen. Now, from the first law, which is probably true, it is possible to get a theory which is definitely true: for, *unless* equal volumes of gases contain numbers of chemical units which are relative to one another as the ratio between small whole numbers, gases could not combine in simple ratios by volume. Combine this now with the rudimentary gas-theory of the preceding paragraph and a very important theory results—the beginning of the kinetic theory of gases. Equal volumes of gases, under the same conditions of temperature and pressure, contain an equal number of *physical units*—the mechanical, pressure-volume things (molecules), and, if the gases are elementary, the molecules contain each a simple whole number of chemical units or atoms. This has been called "Avogadro's Hypothesis" after the man who first enunciated it. It is of the utmost importance to physical chemistry. But it is not an hypothesis, it is not a guess; it is a theory—and true.

With this bit of theory available, we are in a position where it is relatively easy to learn more new truth. The question might arise: How many atoms does the hydrogen molecule contain?—or, in another form: Is the chemical unit of hydrogen identical with the physical unit? One volume of hydrogen combines with one volume of chlorine to form two volumes of hydrochloric acid gas. Then the physical unit of hydrochloric acid contains two chemical units, one of hydrogen and one of chlorine. Let n equal the number of physical units contained in a volume of gas under the conditions of the experiment. Then we have that n physical units of hydrogen combine with n physical units of chlorine to form $2n$ physical units of hydrochloric acid. But the $2n$ molecules of hydrochloric acid contain $4n$ atoms. Then the n molecules of hydrogen plus the n molecules of chlorine must have contained $4n$ atoms. Moreover, the number of hydrogen atoms and of chlorine atoms was equal. Therefore, the hydrogen molecule and the chlorine molecule each contains two atoms. *Unless* each contains two atoms, one volume of one could not combine with one volume of the other to form *two* volumes of a gas whose molecule, from the fact that it is a compound, is known to contain two atoms.

This bit of information, that the hydrogen molecule contains two atoms, is logically of the nature of a theory. So far as I know it has not been called either a law, or a theory, or an hypothesis. It seems less important and less universally interesting than the truth that light is a wave motion. Still it deserves to be pointed out that science is full of these little bits of knowledge which are not merely probable (as far more important laws, like the law of gravitation, are probable), but are true.

In a world where one kind of matter combines with another kind of matter in a definite proportion by weight, where gases interact chemically in simple ratios by volume, where the law that the pressure-volume product for a given quantity of gas is proportional to the absolute temperature is a genuine law, there are chemical units and physical units, or *atoms* and *molecules,* and equal volumes of gases contain an equal number of molecules. And the molecules are themselves made up of a perfectly definite and determinable number of atoms—where the atoms are still taken as chemical units in the same manner that the gram is a unit of mass. But in this world the question can not be answered whether or not atoms and molecules are truly particles—discrete and separable.

The fact that a piece of sugar dissolves in water proves nothing as to the inner constitution of the sugar. It is conceivable that the water merely increases the distribution of the sugar—that it is a medium through which the sugar attenuates itself. Sugar might well dissolve in water without either of them—for all we know—being made up of ultimate particles. The fact that a given quantity of water will hold in solution in stable equilibrium only so much sugar and no more—taken by itself—proves nothing. Iron and sulphur combine in definite proportions when heated together to form a new substance whose properties are different from those of iron or of sulphur, and whose properties are not, in any sense, intermediate between, or a blend of, the properties of the elements from which it is made. But it is not unreasonable to suppose that the iron dissolves the sulphur—that they distribute themselves one through the other—and there seems no reason, *a priori,* why the resulting solution could not have properties wholly different from those of the substances from which it is made. The law that elements combine does not require the theory of the atomic particle. Neither does the law of definite combining weights—for there seems no contradiction in supposing that a given quantity of a solvent will dissolve just so much, no more and no less, of another given substance:

if too much of the substance is taken there will be some left over, undissolved; if too much of the solvent, then there will be some of the solvent that is doing the actual dissolving and some that is left over, inactive. The law of multiple proportions, likewise, does not lead to the theory of individual atoms.

Chemists have long held to the notion that matter is composed of individual particles. For a long time the notion was an hypothesis—an hypothesis suggested by facts such as those just mentioned—but not required by them. It made possible clear mental pictures and neat descriptions of phenomena. It had the value of its pragmatic workings, but it did not possess the truth which attaches to a theory.

It does not matter whether the milkman adds the water to the milk or adds the milk to the water; the result in either case is the same. And in making a pudding the order in which the various ingredients are added is immaterial; if the same quantity of each ingredient is used each time, the same pudding will always result. Thus, if matter does not consist of particles, the order in which several elements combine to form a complex compound does not matter, and whenever the same quantities of the same elements combine the same compound will certainly result.

On the contrary, if compounds are built up of particles of the constituent elements, in a manner similar to that in which children build towers and houses from wooden blocks, then the arrangement of the particles is an important matter: and, just as different arrangements of the same blocks will give different sorts of houses or towers, so different arrangements of the same atoms will give different compounds. That is, it is impossible that there should be more than one compound of a given molecular weight and empirical constitution, more than one compound whose physical unit (or molecule) contains definite numbers of specific kinds of chemical units (or atoms) *unless* matter consists of particles. If two *different* compounds are known of the same molecular weight and constitution, then there is the corresponding theory—which is as true as the fact that the two compounds exist—that a compound is built up of particles and that the molecule itself is a particle made up of particles.

In 1823 Liebig demonstrated that silver fulminate and silver cyanate have the same molecular weight and empirical constitution. Such substances are said to be *isomers,* or *isomeric* with one another. Silver fulminate is a white crystalline solid, soluble in water, and exceedingly explosive. Silver cyanate is white, not soluble in water, not explosive, and is decomposed by strong acids. The discovery that this harmless substance possesses the same composition as explosive silver fulminate created a great impression. In 1828 Wöhler changed ammonium cyanate into urea—and proved incidentally that the two substances are isomeric. Ammonium cyanate had been prepared before only from inorganic sources and urea only from organic ones. Ammonium cyanate is decomposed by alkalis to give the alkali isocyanate and one equivalent of ammonia. Urea is decomposed by acids and by alkalis to give two equivalents of ammonia and one of carbon dioxide. Wöhler's preparation of a typical organic substance from an inorganic one served to break down the artificial barrier between organic and inorganic chemistry. But, with the discovery of isomeric substances, the notion of the atomic particle ceased to be merely an hypothesis and became a theory—it entered into the state where it could be seen by reflection to be true. *Unless* matter consists of particles, there can be no isomeric substances.

II. INDUCTION GIVES US WARRANT TO THEORIZE

With these instances of theory before the mind, it will be well to consider briefly the nature of law and of hypothesis. This will not only make the situation clearer by showing us opinions in science which are not theories; but, more important, it will show us by what right theories can claim to be true.

When I have seen a swan and seen that it is white, when I have seen no swans that are not white, when I see more white swans, then I make for myself the generalization, "All swans are white." This is the inductive method. The opinion that all swans are white is a law. It is *probably* true. It is *true* that it is probable that all swans are white. Mr. Charles Peirce has shown that *true* here possesses the same meaning as it does when we say, "It is true that a given thing can not be both *A* and not — *A*." That is, the probability is not merely one that satisfies our mind; it is a genuine probability—provided always that the swans, upon the observation of which I base my generalization, constitute a fair sample of all the swans. Thus, a law in science, like the law that an alloy of two metals has a lower melting-point than either of the metals taken singly, is as genuinely probable as the theorems of the algebra of logic are genuinely true.[10]

The scientist makes first an observation, and, as the result of it, an hypothesis comes into his mind—"Perhaps all the things of this sort are the same"—then he goes about to make more observations to verify his hypothesis; finally, when he has found a number of instances in accord with his hypothesis and none in disagreement with it, his hypothesis becomes a law. That which was at first a pure guess is now probably true. Hypotheses are used in science mainly as a means of getting at laws, but they are not all so used. There are hypotheses that are nothing but fanciful guesses: their only excuse for being is that they give anchorage to ideas—and, alas, many of them go at present by the name of theories.

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[10] A discussion of Mr. Charles Peirce's contribution to the logic of induction may be found in Professor Royce's article in the "Encyclopedia of the Philosophical Sciences," Vol. I., pages 82 *et seq.*

Professor Royce has pointed out that a *fair sample* is an instance which is not antecedently likely to be in conformity with the hypothesis which is being tested.[11] Mr. Bertrand Russell holds that the simple enumeration of instances—from whatever source, and without any consideration of the antecedent likelihood of their conformity—is sufficient to establish an inductive generalization.[12] He says: "The greater the number of cases in which a thing of the sort *A* has been found associated with a thing of the sort *B*, the more probable it is (if no cases of failure of association are known) that *A* is always associated with *B*. . . . For example, a man who has seen a great many white swans might argue, by our principle, that on the data it was *probable* that all swans were white." "Suppose," he was asked, "that a man has a pair of white swans, and that he has never seen any other swans, and that he uses this pair for breeding; suppose that, as his flock increases, he observes that each of his swans is white; is he correct in generalizing that all swans are probably white?" "Yes, he is," said Mr. Russell. This seems absurd, and I am sure that no scientist who has worked much in the field or in the laboratory would think of making such a generalization. He might say: "It seems probable that color, with swans, is an hereditary characteristic," but surely he would not say, "It is probable that *all* swans are white." Such cases as that tend to persuade one away from the views of Mr. Russell, but there remains still an *argument* against his opinions.

In arguing for the mere enumeration of conforming instances as against the enumeration of instances which do conform, but of which none are antecedently likely to conform, he has said, in effect, "You suppose it probable that a certain instance is likely to be in conformity with your hypothesis. Well, this probability itself is the result of the simple enumeration of instances." "But," one might insist, "not at all, this probability has itself been established by an examination of instances which were not antecedently likely to conform." "Very well," Professor Russell may say, "carry your regress back as far as you like, carry it back even to the beginnings of experience where sense-data are accepted in all naïveté, you come at last to a position where you have no longer means of forming judgments about antecedent likelihood—you are *forced* to rely on the enumerations of instances." Now, I answer, in order that instances may be enumerated for purposes of induction it is necessary that they be in the same class—they must be identical in *this one respect.* On the borderland of epistemology, experiences are supposed which are atomic in their simplicity—they have only one property—they have only one mark by which they may be known. Given two such experiences, they either are identical or they are not. If they are identical, they will not serve as instances to illustrate a more general law—they are useless for purposes of enumeration. If they are not identical, then they are not antecedently likely to conform to any namable law. Clearly, induction depends upon the enumeration of instances, but of instances whose conformity is not to be foreseen.

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[11] There is another way of deriving laws—by the discussion of the standardized experiment, but it seems to me that the experiment is itself *probably* a fair sample of all possibly suitable experiments. The experiment is itself a law. Also I believe that the so-called "facts" of experience are not what they seem to be. They also are the outcome of earlier inductions. The only real facts are the infrangible fragments of data which epistemology starts with. Fortunately, to the little inductions which are epistemologically earliest there correspond primitive theories. I hope to develop this matter in another paper.

[12] Russell, "The Problems of Philosophy," Chapter VI., pages 104–05.

Professor Russell says also "a sufficient number of cases of the association of *A* with *B* will make it nearly certain that *A* is always associated with *B,* and will make this general law approach certainty without limit."[13] Evidently he means that as the number of observed instances increases so the feeling of probability grows and grows until at last it amounts to a practical satisfying certainty. That is psychology, not logic. The interesting thing about certainty in induction is that the law, reached by an examination of *fair samples,* is *certainly* a genuine statement of what is probably true. For, "if we choose partial collections from a larger collection, and judge of the constitution of the whole collection from that of the parts chosen, fixing our attention upon definable characters present or absent, in the partial collections, we are aided toward probable inferences by the fact that there are *more* possible 'samples,' or partial collections, that at least approximately agree in their constitution with the constitution of the whole, than there are samples that widely disagree."[14] It is not merely probable that all *A*'s are *B*'s, where the *probable* is a psychological thing open to discussion; if the law is properly made, *it is true that it is probable.*[15]

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[13] Russell, *loc. cit.,* page 104.

[14] Royce (discussing Peirce), *loc. cit.,* page 86.

III. THEORY AS TRUE AS FACT

A theory in science is a statement of the conditions without which a certain law, or set of laws, could not be even possibly true. Whatever may be the meaning of "probable," surely it is something better than "possible." Suppose, for the sake of exactness, that the law states that, relative to a certain group of phenomena, the group itself being exactly defined on empirical grounds, A is probably the truth. We may then make a proposition, ϕ_1: A is possible, and another, ϕ_2: A is impossible. The difference between them, $\phi_1 - \phi_2 =$ *Conditiones sine qua non.* Let us call these conditions the "Body of Opinion, T." On purely deductive grounds one may say: "Unless such and such were the case A would not be possible; there are these *conditions without which* A could not be." In science the body of opinion, T, embodying these conditions is frequently sought after with eagerness, and, when found, it often comes to be known by the name of a theory. I assert that such bodies of opinion exist among the materials of science. They are often called *theories,* but not always. Nor do all so-called *theories* comply with the conditions here defined—many of them are actually pure guesses. It seems certain that the things which science makes, the end result of all its work, are of three kinds—*laws, pure guesses,* and the sort of bodies of opinion here defined. I propose to call the pure guesses "hypotheses," and the sort of things like the body of opinion, T, by the name of "theories."

If the law is true, if it is certainly true that A is probable, it seems to me to be equally certainly and genuinely true that the facts embodied in T are true also. The truth of the theory follows from the genuineness of the law.

In order that there may be a law, there must be at least one event in accord with it, and wherever there is a single event, even though that event gives rise to no law, there is still the set of conditions without which that event could not be. Single events, experiments, observations, as well as laws, give rise to theories. And if a philosopher is so skeptical as to insist that it is only probable that the observed event exists, so long as he admits that it is a genuine probability, then the facts embodied in the corresponding theory are still true. The man of science, I suppose, will not be disposed to question the existence of the event observed in his laboratory; and, for my own part, it seems to me that the presuppositions which common sense makes in regard to the material world are legitimate theories resulting from simple laws which now lie hidden, for the most part, beneath the flux of our unreflecting complex experience. But that by way of suggestion; it is another story and must be developed at another time.

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[15] The probabilities of the insurance underwriter are not genuine probabilities—they are only probably probable. They are derived from the examination of what are *probably* "fair samples." So it is in general with statistical probability. Fortunately, experimental science deals very little with statistical probabilities. A further analysis of statistical probability is needed.

The science of chemistry is peculiarly rich in theories. Tautomerism and labile bonds, stereochemistry, thermochemistry, and the relative stability of allotropic modifications, will show, I suspect, some interesting things when subjected to a logical study. It needs to be known whether the kinetic "theory" of gases and the ionization theory are true theories or plausible hypotheses. The recent work of the Bragges and of Irving Langmuir will probably lead to important *theories* concerning atoms and the structure of molecules. For the present paper it is enough if I have shown that the chemists' atomic theory (although in the early days of the science it was an hypothesis or overbelief) possesses, in the main, the same sort of truth as any bit of experience. Surely its certainty to-day is beyond the starry dreams of Democritus and Epicurus.

The theories of physics seem to be fewer and less vast than those of chemistry. Perhaps this is because, while chemistry is built upon a theory, the foundations of physics are the presuppositions of common-sense activity. Whether the other sciences contain many theories is important matter for research. It would be interesting to learn how much certain truth there is in the "glacial theory," the "theory" of organic evolution, the nebular "hypothesis," or the hypothesis that "the moon came out of the Pacific Ocean."

Finally, it deserves to be noticed that the theorizing tendency, the tendency to seek deductively the conditions without which a certain phenomenon, or group of phenomena, could not be, is one which has characterized the great advances in scientific knowledge. The advent of a new theory has been the index of the advent of genius. Genius the most genuine has been needed to produce real theories. If, in the foregoing, I have chosen poor instances of theory, it is ascribable to ordinary frailty; but the logic remains—and the fact that the great men of science can, and do, tease out of phenomena this illuminating collateral verity.

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